so good as to look at p. 111 and p. 148, vol. ii. of my "Variation of Animals and Plants under Domestication," he will find a good many facts and a discussion on the fertility and sterility of organisms from increased food and other causes. He will see my reasons for disagreeing with Mr. Doubleday, whose work I carefully read many years ago.

CHARLES DARWIN

Down, Beckenham, Kent, July 1

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THE very ingenious manner in which Mr. Howorth first misrepresents Darwinism, and then uses an argument which is not even founded on his own misrepresentation, but on a quite distinct fallacy, may puzzle some of your readers. I therefore ask space for a few lines of criticism.

Mr. Howorth first "takes it" that the struggle for existence "means, in five words, the persistence of the stronger." This is a pure misrepresentation. Darwin says nothing of the kind. "Strength" is only one out of the many and varied powers and faculties that lead to success in the battle for life. Minute size, obscure colours, swiftness, armour, cunning, prolificness, nauseousness, or bad odour, have any one of them as much right to be put forward as the cause of "persistence." The error is so gross that it seems wonderful that any reader of Darwin could have made it, or, having made it, could put it forward deliberately as a fair foundation for a criticism. He says, moreover, that the theory of Natural Selection "has been expressively epitomised" as "the persistence of the stronger," "the survival of the stronger." By whom? I should like to know. I never saw the terms so applied in print by any Darwinian. The most curious and even ludicrous thing, however, is that, having thus laid down his premisses, Mr. Howorth makes no more use of them, but runs off to something quite different, namely, that *fatness* is prejudicial to fertility. "Fat hens won't lay," "overgrown melons have few seeds," "overfed men have small families,"—these are the *facts* by which he seeks to prove that the *strongest* will not survive and leave offspring! But what does nature tell us? That the strongest and most vigorous plants *do* produce the most flowers and seed, not the weak and sickly. That the strongest and most healthy and best fed wild animals *do* propagate more rapidly than the starved and sickly. That the strong and thoroughly well-fed backwoodsmen of America increase more rapidly than any half-starved race of Indians upon earth. No *fact*, therefore, has been adduced to show that even "the persistence of the stronger" is not true; although, if this had been done, it would not touch Natural Selection, which is the "survival of the fittest."

ALFRED R. WALLACE

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## Our Natural History Museum

IN a few days the country will be called upon to vote 30,000*l.* or 40,000*l.* towards the erection of the new Natural History Museum at Kensington. 7,000*l.* were voted last year for the purpose of drawing up estimates and preparing the site, and our present one at Bloomsbury has become such a crying evil that we can scarcely anticipate a refusal of the grant.

So liberal a sum being offered at the shrine of Science, the community at large will necessarily expect great things of her, and first among all a radical redress of all existing grievances. Yet, if rumour whispers true, the prospects of the future are scarcely so brilliant or pregnant with promises of better things to come as they should be. Plans have been drawn up and decided upon, and the chiefs of the present Natural History Departments have been subsequently consulted as to the amount of space required for the several collections under their charge.

This is itself a faulty commencement, for the building should be constructed for the requirements of the collections, and not the collections cut to the size of the building, and, as might have been anticipated, such policy already threatens to prove productive of disappointment and dissatisfaction. Some departments will profit by the change, while others, including the one mostly needing an elargement of its borders, will absolutely have less than the present amount of space awarded it. We refer to the zoological one, whose present overcrowded and semi-arranged condition is a disgrace to the nation. And yet, on the completion of the present plans, this cramming process is threatened to be still further carried out, though it is to be hoped the voice of opposition and common sense will save us yet from so unfortunate a catastrophe. We hear again that no consideration whatever has been devoted to the subject of a library for the new building, nor a single foot of space allotted to the purpose of constructing one. Such a blunder as this surpasses the first one. The scientific volumes in the present library are in constant requisition by the officers of the various departments to assist them in the determination and arrangement of the specimens. Many of these again are unique or only replaceable at a great cost, and the inconvenience and loss of advantages that will arise to the official staff on being separated from the collection of works they now have access to, cannot be over-estimated. If the Natural History collections must be removed, an edifice suitable for their thorough utilisation, and replete with every convenience for prosecuting scientific research, including efficient laboratories, should be erected.

But to commence at the root of the evil. No progress can be expected under present auspices, or so long as the chief administration of the establishment, and the appointment and promotion of all officers, is vested in the hands of some fifty or sixty trustees, out of whom not more than two can be said to take a direct interest in the promotion of Natural Science. Nor, again, so long as such little discrimination is exercised in the distribution of these officers. Curiosity has prompted inquiries which have elicited anything but satisfying discoveries. We find men with talents for one branch of natural history stationed in departments where their particular talents cannot be utilised; recent zoologists in the geological department, palæontologists in the recent botanical one, and men peculiarly gifted for literary pursuits and without the slightest taste for scientific research, in the former. Taking next the department of Recent Zoology, the inadequacy of the present staff and the ill-proportioned attention that is devoted to particular sections, to the entire neglect of the remaining ones, are painfully apparent. In the Vertebrate division, though abundant room for improvement, there is not so much cause for censure; but on descending to the lower and far more bulky one of the Invertebrates, what do we find? Of a staff of five, two are conchologists, and the remaining three entomologists, while the Crustacea, Arachnida, and the whole of the old group of the Radiates, including the Echinodermata, Molluscoida, Cœlenterata, and Protozoa, are left to shift for themselves, and make way for the necessities of the others. Have we no men in England capable of superintending the arrangement of these neglected classes? or is it that the present remuneration for scientific work, for all but those highest in authority—so slender as to necessitate their utilising every leisure hour in eking out other means of subsistence, and taxing their brains, to the detriment of the amount of work discharged in official hours—deters them from coming forward? At any rate, the evil should be attended to, and the present glaring incongruities abolished. Whether new buildings are erected at Kensington, or the existing ones enlarged, it is absolutely incumbent that the administration shall be thoroughly reorganised. A permanent committee of some dozen eminently scientific men should supply the place of the present host of uninterested trustees, and the staff of officers should be distributed in accordance with the plan adopted in the Paris and various Continental Museums. Each zoological section should have its superintendent, with a number of assistants varying according to its requirements, while one governing mind should assume the responsibility and direct the machinery of the whole; and until such reformation is accomplished, there is no hope of any practical improvements. We do not see why the two large wings of the present establishment, now occupied as residences by the superior officers, should not be converted into exhibition rooms; space enough being reserved for one official residence on either side; and if necessary, additional suitable ones might be rented in the immediate neighbourhood, and the collections thus saved the unavoidable wear and tear of removal, and at the same time preserved in their present convenient position of access to the general public. But the exodus has been decided upon, and the question itself is of secondary importance compared with that of administration. On a future occasion I would direct attention to a few other points.

BATHYBIUS

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## Steam Lifeboats

THE *Globe* of Friday last contained a report of the proceedings of the Committee of the Steam Lifeship Fund, from which it would appear that the subject of the construction of a steam lifeship is seriously contemplated. As one who has for several years given great attention to this most desirable object, perhaps you will allow me to give the results of my labours.

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It has already appeared to me that the object to be attained was not the construction of a lifeship, but rather the fitting of lifeboats with steam machinery, thereby improving their efficiency and diminishing the risk of life, as a boat so constructed could be worked, and that more efficiently, by at most three men, instead of the large number now required to man them. The only means of propulsion which can be applied is, in my opinion, the hydraulic propeller, as, the turbine being enclosed, all risk of fouling pieces of wreck, weed, &c., is thereby avoided. To atrempt to use a lifeboat fitted with a screw or paddle would only be courting danger and disaster. Such being the case, the boat designed by me consists of three tubes, the outer ones being circular, and the centre one in which the propeller works being semicircular, and placed underneath the platform grating connecting the two circular tubes. The three tubes would be turned up and unite at the ends, and would somewhat resemble a whaleboat. The peculiar advantage of the hydraulic propeller when applied in this manner is, that the boat could be turned round on its own centre, and sent ahead or astern by the man in charge by simply turning a handle, without issuing an order to any one, an advantage which I need hardly say is of the very greatest moment under such circumstances as those in which lifeboats are usually employed.

The system of towing lifeboats by means of steam tugs to some point as near as possible to the site of the wreck, is one attended with danger, and the lifeboat, when cast off, is deprived of its means of propulsion at the very time when engine power would be most effective in enabling it to contend with the broken water round a wreck. I remember a case at Bombay, when a lifeboat proceeding to a wreck was towed right under, and the Chinese crew swept out of the boat and nearly all drowned.

Tubular lifeboats, I need hardly say, are no novelty, and the addition of a centre tube to carry the propeller and the steam engine and boiler will certainly not diminish their efficiency.

JOHN FELLOWES

Naval and Military Club, Piccadilly, July 3

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## The Internal Stucture of the Earth

ARCHDEACON PRATT's letter in NATURE for June 22 calls for some remarks on my part. He communicates a few marginal notes written by Mr. Hopkins on a copy of the second part of my "Researches in Terrestrial Physics," which appeared in the "Philosophical Transactions," and seems seriously to regard these curt expressions as judicial utterances beyond which there can be no appeal.

In the first place, I am accused of incorrectly stating the nature of Mr. Hopkins's hypothesis as to the non-existence of friction between the fluid nucleus and solid shell of the earth. The words quoted from my paper as incorrect immediately follow a symbolical expression presented by Mr. Hopkins as the final result of his analysis, and my remark distinctly refers to this mathematical expression, and to nothing else. Remembering that the whole of Mr. Hopkins's mathematical investigations on the internal structure of the earth culminated in the deduction of this very expression, it is well to examine what are the words he uses in the course of his investigations which refer to the existence of friction between the shell and nucleus.

In his first memoir, "Philosophical Transactions," 1839, he says, "and since there will be no friction with the assumed perfect fluidity of the interior matter," p. 386. In his second memoir, I do not recollect that anything about friction is mentioned; but in his third, which summarises the whole of his preceding labours, after presenting the formula already alluded to, he states that it was established on the supposition of "the transition being immediate from the entire solidity of the shell to the *perfect* fluidity of the mass." He afterwards gives reasons for believing that a stratum of imperfect fluid probably exists between the shell and the perfect fluid, and he further uses the words, "Consequently *the assumption made in our investigations of the absence of all tangential action between the shell and fluid* will not be accurately true," p. 43. As my remark refers to these investigations and their immediate result, it is unnecessary to say to whom the charge of inaccuracy may justly apply. In affirming the existence of friction between the shell and nucleus to such an extent as to cause both to rotate as one solid mass, friction between the particles of the fluid is clearly implied; for if no such friction existed, the film of liquid touching the shell and moving with it might slip over the remainder of the nucleus. I have, therefore, been all along at issue with Mr. Hopkins on this point, when I concluded that the rotation of the shell and nucleus must take place as if the whole were solid. Mr. Hopkins declares this conclusion to be "a mechanical impossibility." It is this "impossibility" which has been reaffirmed by M. Delaunay in stronger terms than those I used. It has been shown to be not merely possible, but rigorously true, in a particular case, by an experiment of M. Champagneur, which I have myself recently verified, and it has been further so clearly illustrated in these pages by two correspondents A. J. M. and A. H. Green (May 18, p. 45) as to require no further observation. The coincidence of the axes of instantaneous rotation of the shell and nucleus necessarily follows if the whole moves as a solid mass; and to charge me with implying the coincidence in one of my formulæ is equivalent to charging me with being strictly consistent On this point Mr. Hopkins is of course at issue with M. Delaunay as well as with myself. The next important question referred to on which I totally differ from Mr. Hopkins is that of the form of the inner surface of the shell. If the shell has been gradually formed by solidification from a fluid mass, it is evident that the rate of progressive solidification at the interior of the shell must depend on the rate of refrigeration of the surface of the nucleus. This takes place, and has probably taken place for ages, at an almost insensible rate of slowness, and therefore also the successive additions of matter to the shell's inner surface. Between the perfectly solidified and comparatively rigid part of the shell and the fluid nucleus, the matter on the point of becoming solidified is probably in a pasty or imperfectly fluid state (as Mr. Hopkins has admitted), and it is this matter which is subjected to a moulding action by the changes of shape of the nucleus, as I pointed out in the publication already alluded to. This pasty matter becoming slowly impressed with the shape of the nucleus, and freely yielding to the impression as it passes to the solid state, the more rigid part of the shell, precisely as the outer case of a mould, is saved from strain, and cannot undergo a corresponding change of figure. In the discussion which followed the reading of my communication to the French Academy of Sciences on March 6, it appears from the *Comptes Rendus* that M. Elie de Beaumont made some remarks which illustrate and support this view of the process of formation of the shell. The conclusion to which I was thus led, that the inner surface of the shell could not be less elliptical than its outer surface, was reaffirmed soon after the publication of my researches by an eminent mathematician, the late Baron Plana, of Turin. All this Mr. Hopkins considers as quite inadmissible, and very reasonably, too, in the opinion of Archdeacon Pratt, and all the results deduced therefrom are judicially pronounced to be "valueless." But my conclusion as to the interior ellipticity of the shell is only a necessary deduction flowing from the fundamental principles from which my inquiries start, a principle upon which I am as much at issue with Mr. Hopkins as upon anything referred to in his marginal notes. As this is the really vital divergence between us, a few words of explanation are desirable

The hypothesis of the entirely fluid state of the earth anterior to its present state forms the groundwork of mathematical inquiries as to the earth's figure. The problem, as hitherto treated, always involved an additional hypothesis either openly or tacitly implied, namely, that the distribution of the particles composing the earth underwent no change by the earth's transition from a completely fluid condition to its present state. While Mr. Hopkins tacitly assumed this second hypothesis throughout his investigations, I have reason to believe that it was for the first time rejected in my paper on the "Figure and Primitive Formation of the Earth," which forms the first of my "Researches in Terrestrial Physics." By this step we are at liberty to investigate, with the aid of mechanical and physical laws and the known properties of the earth's materials, the probable arrangement and laws of density of the interior strata of the shell and nucleus. In attempting to do so, I was led to conclusions as to the earth's internal structure widely differing from those of Mr. Hopkins. I have great difficulty in believing that the crude comments on my researches communicated to Archdeacon Pratt, could have been intended to meet the public eye. Long before Mr. Hopkins sent these remarks to Archdeacon Pratt, he wrote to me promising to comment publicly upon my conclusions; and since then an opportunity occurred for pointing out in his presence at a meeting of the British Association what I conceived to be the inconclusive character of his results. Mr. Hopkins promised to reply, but neither this

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